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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Do biomass burning aerosols intensify drought in equatorial Asia during El Niño?" *by* M. G. Tosca et al.

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Reviewer #2 (Jeffrey Reid):

We are grateful for Dr. Reid's careful review. We modified the text of the manuscript to address most of Dr. Reid's suggestions. These suggestions have considerably strengthened the manuscript.

Detailed Response to Reviewer #2 Comments

This is a very well written paper and gets straight to the point. They ran CAM3 with high and low emissions cases and look at differences in the model. Model components are briefly yet adequately explained. Introduction lays out the problem very nicely. They are moderately careful with regard to some of their key conclusions. Not much to argue with in this context. In *short, I wish more papers were written in this general format. I do have a number of issues on the observation end, but in the context of this simple simulation it is not worth getting into.*

However, I do have some issues with the works broad interpretation. These I think are more for the official record in ACPD and less for the model-given the circumstances not much more than they can do. Since the 1997 El Nino event people have speculated on the possible feedback between smoke and dynamics. I still remember as a graduate student at the University of Washington a conversation I had with Conway Leovy he immediately fingered possible emissions feedback through suppressed precipitation. But, it is a very tough problem to get at. As Tosca et al pointed out in the paper, regional emissions are partly phase locked with precipitation patterns and a clear tipping point exists with regard to surface water levels in the peat lands and fire prevalence. Our own work shows similar results of Tosca et al with the neutral and la nina years have similar fire characteristics whereas el nino has massive smoke production. But there in lies the problem. Global climate models have variable success reproducing el nino events. So if there is uncertainty in the model, how can one determine real feedback? If the authors want to go this route, then they have to isolate el nino cases verus normal and la nina cases from their 30 year runs. El Nino periods are already dry and they have to prove it in the model. El Nino impact then ends with the flip of the itcz. Did the model run suggest a delay or advance in this flip?

The purpose of our study was to assess short-term regional climate responses to changes in fire aerosol loading considering atmosphere and surface ocean interactions. In the Methods we've added text in response to this point and also in response to reviewer #1 to clarify that the slab ocean we used with the Community Atmosphere Model precludes us from assessing longer term feedbacks involving ENSO dynamics. Our conclusions are based on the assumption that the mean regional precipitation responses to elevated aerosol concentrations that we observed using climatological boundary conditions for the slab ocean model were also relevant for climate responses to aerosols during El Niño years.

Indeed, what Dr. Reid describes in his review is clearly the next step for the field - to attempt a set of fully coupled ocean-atmosphere GCM simulations with prognostic fire. A key goal of

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this future work would be to assess whether ENSO dynamics change in response to fire-aerosol forcing. The ability of AO-GCMs to simulate ENSO has improved considerably in recent years, giving us hope that this problem is tractable. Clarifying text has been added to Methods section:

We forced CAM3 with monthly emissions of BC and OC from GFEDv2. In one simulation we prescribed GFEDv2 fire emissions from 1997 to represent a high fire (El Niño) year (Fig. 1). In a second simulation, we prescribed fire emissions from 2000 to represent a low fire (La Niña) year. All other aspects of the two simulations were identical, including initial conditions, allowing us to isolate the short-term climate response caused by fire-induced aerosol forcing. We performed two forty-year simulations using these two sets of annually repeating GFEDv2 fluxes. We excluded the first 10 years from each simulation to account for spin-up effects, including adjustments to the hydrologic cycle. In our analysis we defined fire-induced climate anomalies as the difference between the high and low fire simulations.'

'The slab ocean model responded to the different aerosol loadings in the two simulations, causing changes in SSTs. Large scale ENSO-related conditions and circulations are not represented in the boundary conditions, which were all climatological except for the fire-induced aerosol emissions. Aerosol forcing changed the surface energy budgets of the two simulations but not the prescribed, climatological ocean mixed layer vertical heat fluxes. The resulting SST anomalies dynamically interacted with the atmosphere by means of radiative and turbulent energy fluxes. Thus, the climate responses described here should probably be interpreted primarily as the short-term response (over a time span of several months) of the atmosphere-surface ocean system in equatorial Asia to aerosols from El Niño fires. An important next step (as described below in the discussion) is to repeat this analysis with a prognostic fire emissions model and a fully coupled ocean-atmosphere general circulation model to examine longer term fire–ENSO feedbacks mediated by changes in ocean circulation.'

In the discussion we return to this issue with a paragraph starting on line 378 that starts with the sentence: "An interesting question raised by this analysis is whether fire-emitted aerosols

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influence El Niño dynamics over a period of years to decades." We follow by describing how the El Niño-induced fire forcing is phase locked with ENSO and how this forcing differs from other agents such as greenhouse gases and the South Asia aerosol cloud.

What they are really showing in this paper is not the enhancement el nino, but rather to local convection. Here lies another problem. The orographics of the maritime continent are brutal, and subject to many mesoscale flows. I am sure the Randerson's group is aware of this as half the staff was standing in front of my students paper last agu where she explained it (sic) to them. Mesoscale models cannot simulate controlling orographic and MJO precipitation. So who much can we trust CAM3 at 2.5x2.5 degrees? Is a 10% decrease in modeled precipitation really something we should get excited about? It would be helpful if the authors spent an extra page demonstrating that they understand the nature of the meteorology of the region. This should then be linked back to in their discussions.

We have added the following paragraph at the end of the Discussion to point out to the reader the complexities of the regional meteorology, and to further clarify for the reader possible limitations to the study as a result of our use of a coarse resolution climate model:

'The ability to accurately simulate the mesoscale meteorology of the Indonesian archipelago is limited by the coarse resolution of CAM3 and the unique geography of the area. The region receives over 50% of its rainfall from convective activity (Mori et al., 2004; Chen and Houze, 1997), and interactions between island topography and open ocean fuel diurnal and regional variations in precipitation. As a result of the daytime sea breeze, rainfall rates on land are greatest during the late afternoon coinciding with the timing of maximum boundary layer height (Mori et al., 2004; Schafer et al., 2001; Chen and Houze, 1997). Most of the island rainfall is the result of isolated convective clouds. After sunset, convection over land aggregates to form mesoscale convective complexes, which move over the ocean and increase offshore rainfall rates during the early morning (Mori et al., 2004; Williams and Houze, 1987). During the Northern Hemisphere winter, the diurnal amplitude of convection can be three times greater over the islands than the adjacent ocean, owing to the smaller heat capac-

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ity of land and subsequent variations in low-level instability (Yang and Slingo, 2001). The active phase of the Madden-Julian Oscillation (MJO) suppresses the amplitude of the diurnal cycle (Sui and Lau, 1992). In turn, the Indonesian archipelago alters the strength and speed of the MJO. Neale and Slingo (2003) conclude that even with a threefold increase in horizontal resolution there is little improvement in regional rainfall simulation in a global climate model. Our results suggest that an increase in absorption and scattering from smoke aerosols weakens the lapse rate. We acknowledge the difficulty of accurately representing the spatial and temporal pattern of the resulting precipitation decreases in our model due to challenges in simulating mesoscale processes in the region.'

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23319, 2009.

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